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DISCUSSION

CAUSE AND GROUND. A REPLY

DR. BOSANQUET'S very interesting discussion of cause and ground,¹ in that it was elicited by an article of my own, calls for some reply. I shall, however, endeavor to confine my comments to two points directly raised in my own article: (1) the difference, if any, between Dr. Bosanquet's ideal of methodology and my own, (2) the question whether his treatment of cause and effect is, in fact, a contribution towards either ideal. The other questions raised in his discussion, the relation between Hegelianism and pragmatism, the larger problem of the philosophy of cause, the general utility of tautology, I shall not attempt to discuss.

The first of these I pass by because I do not think I can rightfully be called a pragmatist;² certainly I am not competent to defend pragmatism against the metaphysical school of thought. The second it will be well to defer because, should it ever come my way to make a contribution to the philosophy of cause, which is doubtful, I should prefer to do so *de novo*, rather than incidentally in a particular controversy. Nor do I think the general utility of tautology relevant to the matter in hand. The widening of the ground of discussion certainly enables Dr. Bosanquet to create a pleasing illusion of Platonic support, but the point with which we are concerned is not whether an apparent tautology ever actually conveys new information, but whether a useful purpose is served by Dr. Bosanquet's special treatment of cause and ground.

1. In Dr. Bosanquet's conception of methodology, as expressed in his third paragraph, I find little with which to disagree. But what precisely is the meaning of his unnecessarily emphatic and repeated assertion that it is not the function of philosophy "to teach scientific men their business"? What *is* the business of a scientific man? If Dr. Bosanquet means to tell us that every worker has his own work, and that the philosopher is not required to enter the laboratory, or

¹ This JOURNAL, Vol. VII., No. 16, p. 438.

² On this point it is as well to give some brief explanation. My own position is that of one who approaches philosophy from the scientific side, with decided views of my own, which I am endeavoring, in every possible way, to put before the philosophical world. With regard to the controversies that divide the philosophical world, I usually find, not having myself passed through the philosophical mill, that they concern matters which are to me of secondary importance, or of which I have very imperfect knowledge. My principal ground of sympathy with what is called pragmatism is the demand that philosophy should have a real practical bearing on science and on every-day life.

to inform the chemist and the physicist how to carry out an experiment, surely that is only a truism. But if he means that the scientist and the philosopher each inhabits a universe of his own, that they deal with entirely distinct series of problems, that their spheres never meet, that their problems are in no way interrelated, that controversy, cooperation, or discussion between them is impossible or out of place, the falsity of the statement is equally apparent.

The point of my essay, of which a practical example has just been published,³ is that, when the philosopher undertakes to investigate the principles of scientific method, the investigation should be one which the scientist should not be able to ignore. Let me illustrate my meaning by a simple analogy. It is clearly and obviously not the function of the science of theoretical mechanics to teach the mechanical engineer his business. The mathematical mechanical theorist investigates the principles which underlie the complexities of all types of machinery. He does not attempt to tell the working engineer how to build a bridge. Yet it will be universally admitted that, if the working engineer is in no way more competent to design a bridge by reason of his study of mechanics, there must be something seriously wrong with theoretical mechanics. Similarly, we are entitled to assume that if, as appears to be the case, the study of the philosophical analysis of scientific method in no way fits the scientist to deal more adequately with the problems that are presented to him, there is good *prima facie* evidence that the currently accepted principles of methodology are erroneous or inadequate.

2. That this criticism is applicable to the current metaphysical⁴ treatment of cause and effect, most explicitly stated by Dr. Bosanquet, no one, I think, will deny. Nor am I able to see that either his very interesting explanation, or his imposing array of authority, necessitates the withdrawal of what I have said.

As I stated in the passage which Dr. Bosanquet criticizes, there is a sense in which we can not fully explain anything until we have explained everything: "No section of phenomena is in reality isolated, but all take their place in the greater cosmic unity." On this plane it is clearly illegitimate to speak of particular causes and effects, in that every phenomenon is in some respects unique, and in that everything is in some way connected with everything else. This is clearly

³ This JOURNAL, Vol. VII., No. 18, p. 481.

⁴ I substitute the term *metaphysical* for *intellectualistic*, which latter term Dr. Bosanquet thinks inapplicable to his logic. I used the term, in default of a better, to express my opinion that his treatment shows a tendency to too great abstraction.

Professor Clifford's meaning⁵ in the passage to which Dr. Bosanquet refers, and *if* it is all that Dr. Bosanquet means, he is open to the taunt, sometimes levelled at philosophers, of telling us what everyone knows in language which no one can understand. As I stated before, "the metaphysical unity is an assumption which antedates the whole investigation."

To do justice to Dr. Bosanquet, he does, in reality, mean more than this, and it is because he means more that I cited his treatment of cause and effect as an example of the deficiencies of the current methodology. For the special interest and value of the conception of cause and effect (though not necessarily of all descriptive science) is that, notwithstanding the metaphysical uniqueness of every phenomenon, and the metaphysical unity of the whole universe, certain events can, in fact, be repeated. The interest lies in the repetition not in the uniqueness. And it was because of the recognition of this that the treatment of Mill was superior to that of his successors. On this plane you certainly do, as Mill⁶ clearly pointed out, have to reckon with "plurality of causes," and you do, in many cases, indeed usually, find the cause clearly and obviously preceding the effect in time. The horse moves before the cart, if only by a minute fraction of a second. The point of my criticism was that, if the philosopher undertakes to write a philosophy of cause, he must start by showing the utility and the applicability of the conception, and by tracing its development in the various sciences. From this standpoint, Dr. Bosanquet's explicit statement that "he does not in the least imply that men of science ought to use other ideas than they do use at particular points in their work," is a distinct step in advance, as is also his admission that in chemistry the concept is "fully in place." When the philosopher has accomplished this preliminary, he should then show that the contradiction between the popular and scientific conception of cause and effect, and the metaphysical unity, the assumption of which underlies all scientific investigation, is only apparent. Now the empirical investigation of the idea of cause in the various sciences, or, in other words, the putting of "methods, processes, ideas,

⁵ With the best will in the world, I am unable to see the exact relevance of Dr. Bosanquet's reference to the "descriptive" movement in modern science. This development, if regarded as a metaphysical theory, I will say in passing, I believe to be mistaken, and I have in a previous article pointed out some of the errors of one of its most prominent representatives (see *Mind*, January, 1910, particularly p. 52), but with this, as with other matters, I should prefer to deal, if at all, explicitly. Here I would merely remark that a totally different metaphysical theory, which dispenses with the idea of cause for the purposes of practical science, is hardly a support to Dr. Bosanquet's expressed desire to put the idea in its place.

⁶ "Logic," Vol. I., p. 504.

in their place'' is the element that I miss in his logic and in that of others of his school. My incidental criticism, to which I still adhere, is that while it ''maintains its relation to the metaphysics from which it has been obtained, it has lost its connection with science.'' I desire to add, however, that the (admissions and) explanations to be found in Dr. Bosanquet's discussion, if they are more fully developed in subsequent work, will go some way towards remedying the deficiency.

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CAUSE AND GROUND. A REJOINDER

MR. SHELTON has kindly shown me his paper, and suggested that I should make an addendum. I fear it would be difficult for us to come to an agreement; for Mr. Shelton, as I judge from his foot-note 2, approaches philosophy rather from the outside, and demands that its treatment of its material shall be adapted to his questions. And I think it very likely that it will not be able to satisfy him, even if, as I believe, its treatment and material are fuller than he has trained himself to recognize. Offering more than is easily grasped causes misapprehension, as well as offering too little.

The question is, he says, whether such a treatment of causation as mine serves a useful purpose. Well, what is a useful purpose? Mine, in this case, is to satisfy a great human interest by helping to clear up the nature of knowledge. His, I fear, is to subserve the progress of natural sciences. I see nothing more useful in the latter of these than in the former. I do not think Mr. Shelton would say ''useful = conducive to 'practical' interests.'' If he did, we should have to drop our discussion till we could talk out pragmatism. I did think he had leanings that way because of his demand for practical science from a branch of philosophy, and that was why I held pragmatism relevant.

But he may take me on the true ground of philosophy, and say that I *don't* help to clear up the nature of knowledge. Now I think that he really has not quite seen how entirely relevant my argument was. For he supposes that my references to tautology, and to the descriptive view of science, and to what he thinks ''a totally different metaphysical doctrine,'' the doctrine of ground, are irrelevant. But here, I submit, he has not quite got inside his subject. For the point lies in the unavoidable transformation of the conception of ''cause and effect'' according to the phase of common sense or of science with which we may be dealing. And all the points I referred to lie